

## Comments About Morris's Paper

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Morris's paper (1993) would have benefited from distinguishing between ontology and epistemology and discussing mechanism and behavior analysis in these two domains separately. Overton and I also did not explicitly make this distinction, perhaps contributing to our partly erroneous classification of Bijou and Baer's position as mechanistic (Overton & Reese, 1973; Reese & Overton, 1970). We correctly noted that their position is consistent with the "reactive organism model," which reflects a mechanistic ontology, but we should not have implied that it is consistent with a mechanistic epistemology. We did not discuss the worldview Pepper (1942) called *contextualism*, because at that time it had little if any influence within developmental psychology, which was the topic of our analysis.

The relevance of these issues to Morris's paper is discussed in the rest of this commentary. A preliminary point is that *behavior* and *response* are used synonymously in the commentary, as they are in Morris's paper, because even though *response* implies "reply to" or "elicited by" a stimulus (Reese, 1986b, among other commentators), substituting *behavior* for *response* risks confusion in traditional phrases such as "response class," " $S^D$ -R- $S^R$ ," and the like.

### Ontology

An ontology involves assumptions about the nature of reality, that is, assumptions about what exists and how whatever exists operates or functions. Whether or not Bridgman—as quoted by Morris via Skinner—was correct in associating these assumptions with "faith,"

major philosophers of science identify them as part of the hard core of presuppositions that are not directly testable but that scientists nevertheless adopt (e.g., Feyerabend, 1978, p. 31; Kuhn, 1970; Lakatos, 1978; Laudan, 1977). Although a hard core is not *directly* testable, a hard core that leads nowhere, or nowhere interesting, is eventually abandoned. Thus, although an ontology is adopted by presupposition, it is retained on the basis of evidence: It is abandoned if it does not provide a useful, fruitful, effective "model of the organism" (e.g., Reese, 1986a, p. 169).

The ontology adopted by behavior analysts includes, among other things, concrete instances of the elements in the three-term contingency—stimuli and responses and, implicitly in many analyses, an organism that receives the stimuli, performs the responses, and has had a specific history of stimulation and responding. This ontology does not include disembodied, Platonic ideal forms; all mental events that it includes are assumed to be embodied materially as particularized qualities. This ontology is formally consistent with the ontologies of the "natural sciences" (biology, chemistry, and physics, according to usual definitions), but it does not include the specific elements of these sciences, such as enzymes and DNA, chemical elements and hydrocarbons, and subatomic particles and energy fields.

Several writers have concluded that this ontology—this model of the organism—is consistent with the mechanistic worldview defined by Pepper (e.g., Baltes & Reese, 1977; Hayes, Hayes, & Reese, 1988; Marr, 1992; Overton & Reese, 1973; Reese, 1982a, 1986a; Reese & Overton, 1970). This worldview is elementaristic, but the only kind of reductionism it entails is reduction of wholes to the elements that constitute them. Thus, the mechanistic world consists of

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elements that are described (defined) in different ways in different sciences, and nothing in the mechanistic worldview demands reduction of the elements of one mechanistic world, such as that of behavior analysis, to the elements of another mechanistic world, such as that of biology, chemistry, or physics. In short, the mechanistic ontology of behavior analysis does not require the kind of reductionism that, as Morris correctly points out, is rejected by behavior analysts. The elements are stimuli and responses, not enzymes, DNA, chemical elements, hydrocarbons, subatomic particles, energy fields, or any other elements from a science other than behavior analysis.

### *Epistemology*

Epistemology involves assumptions about knowledge: What is known and how is it known? Or, what is knowledge and how is its truth established? The epistemology adopted by most behavior analysts is not consistent with the mechanistic worldview. Rather, as pointed out by Morris and others, it is consistent with the worldview Pepper called *contextualism* (Morris gives relevant references in his second footnote). An epistemology is "the philosophy that defines adequate methodology" (Reese, 1986a, p. 169) and that makes "the rules of the [science] game" undogmatic (Reese, 1982a, p. 154). Much of Morris's argument, that behavior analysis is not mechanistic, is actually an argument that the behavior-analytic epistemology is not mechanistic. I agree, but the point can be made more convincingly, I have come to believe, by referring explicitly to the epistemology of behavior analysis.

### *Meanings of Mechanism*

The everyday definitions of mechanism that Morris discusses are not relevant to a philosophical or scientific analysis of behavior analysis. The other definitions that he discusses are examined in the present section in the same order as in his paper.

*Zuriff's definition.* According to the def-

inition of mechanism that Morris quotes from Zuriff, mechanism denies "the causal intervention of consciousness, spirit, or a soul." This definition is about ontology; but although it refers to an assumption that is indeed made in all *materialist* ontologies, this assumption is not made in some *mechanistic* ontologies. The concepts of consciousness, spirit, and soul have no counterparts in any materialist ontologies, which assume that only matter and matter-in-motion exist; but they do have counterparts in mechanistic ontologies in which the maker-of-the-machine is taken seriously and is assumed to have a nonmaterial existence. An example—perhaps the only example—of the latter is the assumption of God's will as an antecedent determinant in theological mechanism (Moxley, 1991; Reese, 1992).

Granted, complaining about the failure to distinguish between materialism and mechanism is a quibble, given that the maker-of-the-machine is not taken seriously and materialism is adopted in all modern mechanistic sciences; but nevertheless, the implicit equating of materialism with mechanism is an error because materialism is also consistent with contextualism. In any case, Morris leaps from ontology to epistemology in commenting that the negative assumption denying such nonmaterial entities is equivalent to a positive assumption embracing "naturalism." Naturalism is an epistemological assumption, asserting that all phenomena can be adequately explained by scientific laws (its detractors call it "scientism").

*Malone's definition.* In Morris's quotations of Malone, the denial of nonmaterial causes is equated with determinism. However, determinism is an ontological assumption when it refers to the nature of cause-effect relations and is an epistemological assumption when it refers to explanation. Epistemologically, it is a denial of final and chance causality, but it does not require denial of the nonmaterial antecedents assumed in theological mechanism and in versions of "objective idealism" in which the telos is conceptualized as the beginning as

well as the end (Hegel's philosophy is an example). Evidently, then, Malone erred in equating mechanism—actually, materialism—with determinism.

*Mechanism as natural science.* Morris says that mechanism means “natural science”; but as usually defined, *natural science* refers to a science that is consistent with materialism, not necessarily with mechanism.

*Pronko's definition.* The definition of mechanism that Morris quotes from Pronko requires that phenomena be explained “in terms of the principles of physical mechanics.” This definition refers to epistemology, but not the mechanistic epistemology of classic mechanistic psychologies, such as Watson's theory reducing thinking and other complex processes to simple behaviors (Bergmann, 1956), Loeb's (1912/1964) theory reducing complex behavior to tropisms, Kuo's (1921) theory reducing instincts to learning, and stimulus–response (S–R) learning theories (e.g., Spence, 1956), including, for example, Munn's (1965) learning-based theories of psychological development. Thus, although behavior analysis is not mechanistic in this sense, neither is any other psychology.

*Runes's definition.* Morris quotes a definition by Runes that again confuses materialism and mechanism, and ontology and epistemology. Also, according to Morris, Runes's definition indicates that mechanism is atomistic, elementaristic, associationistic, and essentialistic. Morris argues that behavior analysis does not fit this definition of mechanism because behavior analysis deals with functions and process and is not atomistic, elementaristic, associationistic, or essentialistic “in these senses.” These are too many “istics” to deal with in a brief commentary, other than to note that S–R learning theory, which is certainly mechanistic, is elementaristic but not atomistic, is associationistic, and is realistic rather than essentialistic. That is, Runes's definition does not fit classic mechanistic psychology; therefore, its not fitting behavior analysis is uninformative with respect to whether or not behavior analysis is mechanistic.

A relatively minor point is that Morris says that the subject matter of behavior analysis is described in the grammar of verbs (i.e., gerunds) such as “behaving,” “constructing,” and “thinking” rather than in the grammar of nouns such as “behavior,” “constructs,” and “cognition.” Actually, however, in most behavior-analytic reports, nouns rather than gerunds are used to refer to behavior, and when the gerund-form is used at all, it is usually used as a gerundive, modifying a noun, as in “eating behavior,” “smoking behavior,” and “key-pressing behavior.” Furthermore, the gerundive seems to be used often to identify a form, structure, or topography or to identify a *class* of forms, structures, or topographies.

*Wolman's definition.* The definition Morris quotes from Wolman includes the assertion that in mechanism, “Free will, motivation, and purpose are denied as important variables in attaining ends.” The juxtaposition with “free will” and “purpose” seems to indicate that “motivation” was denied any teleological function, which is true; but motivation—or “drive,” as it was also called—was a crucial variable in mechanistic S–R learning theories (e.g., Brown, 1961; Spence, 1956, chapter 6). Thus, although behavior analysis is not mechanistic according to the “motivation” part of Wolman's definition, neither is any other psychology.

Two incidental points: (a) I agree with Morris that “free will” cannot be a cause of behavior in behavior analysis. The reason is that it does not exist in the behavior-analytic ontology. However, (b) I disagree that “purpose” cannot be a cause of other behavior. For example, when a purpose is stated as part of a rule and when the organism has had a relevant history with respect to the rule, the purpose can cause other behavior. Furthermore, its being a dispositional concept is irrelevant, because dispositional concepts are “explanatory” when they appear in a law that is used to explain some phenomenon.

*Philosophical definitions.* Morris cites some philosophers who have classified behavior analysis as an S–R psychology

with only one behavioral process—reflex or respondent conditioning—and who therefore have classified it as mechanistic. He points out that they erred in this classification, and he concludes that behavior analysis is therefore not mechanistic. However, (a) no mechanistic S–R psychology admitted only one behavioral process. For example, S–R learning theory, which is probably the best articulated mechanistic S–R psychology, admitted two conditioning processes (classical or Pavlovian conditioning and instrumental conditioning) as well as other behavioral processes (generalization and motivation are examples). Thus, the fact that behavior analysis is not in the philosophers' class of "mechanistic" S–R psychology is irrelevant to the issue of whether behavior analysis is mechanistic in any important sense. (b) Not all S–R psychologies are mechanistic; for example, Tolman's is contextualistic (Pepper, 1934). Thus, the question of whether behavior analysis is consistent with S–R psychology is irrelevant to the question of whether behavior analysis is mechanistic. (c) Morris implies that operant behavior is inconsistent with mechanism. I have argued elsewhere against this notion (Reese, 1986a, pp. 170–172).

*Other definitions.* Morris cites dictionaries of psychology in which mechanism is identified with reductionism, which is an ontological principle when it refers to elements that are assumed to exist and is an epistemological principle when it refers to explanations of epiphenomenal wholes. For example, Morris quotes Reber as referring to reduction of psychological principles "to basic principles of physics and physiology." Three points are relevant: (a) Some behaviorists, including Skinner (1974), posed this kind of reduction as an ideal, and some, including Bugelski (1982), Hebb (1949), and Hull (1943), even gave physiological-sounding names to some of their intervening variables and hypothetical constructs. However, this "neurologizing" is not truly reductionistic (Reese, 1982b); it is lip-service reductionism and was exhibited not only by some mechanists but also by some nonmechanists (such as

Rignano, 1923, p. v). Thus, the kind of reductionism Morris attributes to mechanism was never taken seriously in behavioral research and theory. (b) Morris conflates "elementarism, associationism, and reductionism," but as already noted, mechanism is not reductionistic except in the sense of elementarism, and even then it refers to elements only within a domain, not across domains. As noted herein in the section entitled *Ontology*, behavior analysis is also reductionistic in this sense. (c) Morris's conflation of associationism with elementarism reflects historical fact: Classical associationism and S–R behaviorism were mechanistic, hence elementaristic, and were also associationistic. However, if (as in classic British associationism) associationism is understood to mean the reduction of mind (or any complex phenomenon) to an "and-summation" kind of combination of elements, as opposed to a chemical compound kind of combination, then associationism is not a necessary by-product of the mechanistic worldview.

*Conclusion.* Morris's concluding point on meaning and definition is not altogether clear to me, but it seems to reflect the well-known superiority of contingency shaping to rule governance with respect to language. If so, the point is not limited to behavior analysis; it is the learn-by-doing principle, which is endorsed not only by mechanistic S–R learning theorists but also by dialectical materialists (e.g., Kautsky, 1908/1953, pp. xii–xiii; Lisina, 1985, pp. 8–9; Mao Zedong, 1937/1965), dialectical idealists (Annas, 1981, pp. 276, 292; Ong, 1958, p. 197; Piaget, 1983), and contextualists (Kozulin, 1984, p. 131). One implication is that one should stay close to the sources of the issues; thus, to find out how to define "mechanism," one should read the actual reports of actual mechanists, such as Hull and Spence, rather than the secondhand discussions in philosophies, dictionaries, and glossaries.

### *Model Issues*

The advice at the end of the preceding paragraph is especially relevant to Mor-

ris's discussion of the six corollary model issues that Overton and I discussed, because Morris misinterprets all six of them.

*Elementarism versus holism.* Mechanism is elementaristic in the sense that a whole is conceptualized as completely reducible to interrelated parts; organicism is holistic in the sense that this elementaristic principle is denied. Overton and I (Reese & Overton, 1970) contrasted the holistic assumption with an extreme form of elementarism, a form that was used in "physicalistic behaviorism" but not in "methodological behaviorism," as defined by Bergmann (1956). The physicalistic behaviorists wanted to describe responses in terms of molecular elements—muscle twitches and glandular squirts—but although their elements have sometimes been attributed to Watson and the methodological behaviorists, the fact is that most S-R behaviorists defined responses in the same way as Skinner did. For example, Spence (1956) discussed the distinction between molecular *movements* (or *actions*) and molar *acts* and commented,

For the most part learning psychologists employ this second class of response concepts. Certainly all the responses involved in instrumental conditioning and selective learning situations would be classified as acts rather than movements, and contrary to common belief, even the responses employed in classical conditioning appear to fall into this category. . . . As long as the measuring operation defining the response variable ignores variations in the pattern of muscular action involved and specifies the response only in terms of some environmental outcome or change in the organism-environment relation, it falls into the class of responses called acts as distinguished from movements. (pp. 42–43)

Spence then discussed at some length (pp. 43–45) the concept of response chains, specifically citing Keller and Schoenfeld (1950) and concurring with their comment that the segmentation of such chains is arbitrary because the behavior is actually a continuous flux. (Spence did not cite a page; their p. 202 is relevant. He could also have cited Skinner, 1953, p. 224.) The continuous-flux assumption is ontological; but when the mechanistic model is adopted, this ontology is replaced by the elemental ontology.

Thus, behaviors defined in a molar way

constituted, together with stimuli, the "elements" into which more complex psychological phenomena, such as discrimination learning and paired-associates learning (Spence, 1956, chapter 7) and personality and psychotherapy (Dollard & Miller, 1950), were analyzed. Defining responses functionally does not contradict this elementarism; Morris therefore errs in concluding that on this issue, behavior analysis is not mechanistic.

*Antecedent-consequent versus structure-function.* Morris misinterprets the antecedent-consequent assumption; it actually involves accepting material and efficient causality, ignoring formal causality, and rejecting final and chance causality (in modern versions of Aristotle's senses of these causes). Morris points out, correctly, that behavior analysts reject this assumption, but the point is irrelevant because the assumption is epistemological.

Morris raises the issue of essentialism in connection with this principle, but I think he has a different concept of essentialism from mine. I think he uses "essentialism" to mean that every abstract concept reflects some physical or structural property of real objects. My understanding is that it means that every concept, whether abstract or concrete, reflects some real property of real objects, and furthermore, contrary to Morris, the property that is reflected is not necessarily either physical or structural but can be functional. According to my understanding, behavior analysis is consistent with the essentialism of the mechanistic ontology.

*Behavioral change versus structural change.* The questions here are: What changes during development? What is the nature of these changes? What is the direction of these changes? In mechanism, the elements (e.g., stimuli and responses) do not change but their relations change; the changes do not reflect dialectical leaps (discontinuities) in structures but rather are continuous; and the direction of change is determined not by final causes but by efficient causes. If the function or role of a response changes, it changes not

because of changes in structure but because of efficient causes. I see no inconsistency here between behavior analysis and mechanism.

Morris adds another issue—changes in relations among formal stimulus and response elements versus changes in functional relations among stimulus and response classes. Four counterpoints can be made: (a) As already noted, mechanism is not essentialistic in this way. (b) Behavior analysts in fact deal with changes in the number, strength, and association of stimulus and response elements, which they refer to as changes in response rate, but this point is also not relevant. (c) Mechanism embraces changes in functional relations among stimuli and responses as well as changes in formal relations. (Incidentally, “formal” is used here to mean “of or pertaining to form”; it does not refer to “formal logic.”) (d) Nobody has ever *observed* a stimulus class or a response class. These concepts refer to inferences, or as behavior analysts might prefer to say, they refer to evidence about stimulus and response functions. Specifically, a stimulus class as such is not observed; it is a shorthand phrase used when certain stimuli are found to have the same function with respect to responses. Similarly, a response class as such is not observed; it is a shorthand phrase used when certain responses are found to have the same relation to stimuli (usually without direct conditioning of the relation for some of the responses). Thus, although stimulus and response classes are not ontological concepts in mechanism, they are also not ontological concepts in behavior analysis. They have the same epistemological status as such intervening variables as *habit strength* and *drive strength* in S–R learning theory. Therefore, the purported emphasis of behavior analysts on stimulus and response classes is not relevant to the issue of whether the behavior-analytic ontology is consistent with mechanism.

This argument hinges on what is meant by “class.” Morris uses as examples “initiating a conversation” and “social reciprocity”; but by the definition used in classic mechanistic psychologies, these

are instances of “acts” in Spence’s sense. Given that Skinner used the same definition of responses, the point illustrated by Morris’s examples can be interpreted to refer to simple stimuli and responses rather than to stimulus and response classes in the strict behavior-analytic sense. So interpreted, Morris’s point remains irrelevant to the issue of whether the behavior-analytic ontology is consistent with mechanism.

*Continuity versus discontinuity.* The continuity–discontinuity issue refers to whether all changes that occur are traceable to antecedents (continuity) or some changes are emergent (discontinuity). The issue does not refer to the predictability versus unpredictability of changes, nor to gradual versus saltatory changes; it refers to whether the state of an object or event after change has occurred can be deduced from the state of the object or event before the change occurred (for discussion, see Overton & Reese, 1981). According to mechanism, the deduction is in principle possible; according to organicism, it is in principle impossible. However, the principle is epistemological and therefore is not relevant to the issue of whether behavior analysis is ontologically mechanistic.

*Unidirectional and linear causality.* In the so-named section in his paper, Morris conflates two issues that Overton and Reese (1973) called “unidirectional versus reciprocal causality” and “linear causality versus organized complexity.” The issues are complex, but they can be put simply as follows: According to the mechanistic conception of causality, the cause–effect relation between two events involves efficient causality and elementarism; it is a one-way antecedent–consequent relation, and multiple causes can interact only in the additive sense of interaction in an analysis-of-variance model (Overton & Reese, 1973). In contrast, the organismic conception involves final causality and mutual interpenetration of events such that neither is identifiable as antecedent or as consequent to the other. These issues are epistemological, and Morris’s argument that the understanding of causality in behavior analysis does

not fit either of these views is correct, because the behavior-analytic view is more consistent with contextualism than with mechanism and organicism.

### Conclusions

Morris's section on the behavior-analytic concept of "the other one" (i.e., the subject matter) should deal with ontology, but it actually deals with a mixture of ontology and epistemology. His arguments that behavior analysts do not view "the other one" mechanistically turn out to be questionable when they refer to ontology and turn out to refer to epistemology when they are correct. Having already far exceeded a reasonable page allotment, I must forego commenting on Morris's section on "the one" and his concluding section, other than to note that issues about "the one" (i.e., the researcher) are epistemological, and unless the behavior-analytic ontology is distinguished from its epistemology, only confused answers can be given to the question of whether "behavior analysis is essentially mechanistic in its science, practice, or philosophy."

In short, Morris's paper does not effectively challenge the conclusion that behavior analysis is consistent with a mechanistic ontology (and a contextualistic epistemology).

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